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The Effect of Enlistment Bonuses on First-Term Tenure Among Navy Enlistees

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with
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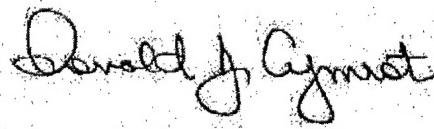


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14. ABSTRACT: While enlistment bonuses (EBs) have traditionally been used to affect accession decisions, it seems likely that offering a recruit a bonus that is payable at the end of training could also reduce attrition. This study attempts to assess the relationship between the size of the enlistment bonus offered a recruit and the likelihood that the recruit attrites-holding all else constant. Following recent studies of enlistment incentives, we employed non-experimental data (data generated from the administration of the EB program) to explore this relationship. Using this type of data creates an empirical challenge because both the size of enlistment bonuses and attrition behavior are likely to be substantially affected by unobserved variables. While there are various empirical techniques to control for the effects of unobserved variables under specific circumstances, we find that these methods have only limited applicability to the current analysis-that is, they can only control for some of the effects of omitted variables. Among our empirical findings, we find limited evidence that enlistment bonuses reduce attrition. While we believe that these statistical results are not substantial enough to guide policy, we suggest that they are sufficiently compelling to justify the Navy pursuing experiments on the issue.		
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Summary¹

Background

Over the last five years, the Navy has greatly increased its use of enlistment bonuses (EBs) as a tool in military personnel management. The Navy's budget for EBs rose from about \$15 million in FY97 to just under \$100 million for FY02. This growth reflects the difficulty the service had in meeting its endstrength and manning objectives over these years. The Navy has been eager to assess the effectiveness of this recruiting incentive, and has recently initiated a number of studies to evaluate the impact of EBs on various aspects of the career decisions of enlisted personnel.² The current analysis, which is undertaken for the staff of N13 (the Navy Personnel Plans and Policy Division), examines the influence of EBs on Navy enlistees' attrition behavior during the first term of service.

Although the principal purpose of EBs is to influence *enlistment* behavior, it seems likely that offering a recruit a bonus that is payable at the end of training might have a secondary effect of reducing *attrition*. If we could hold constant all other factors, we would expect that recruits offered larger bonuses would remain in the service longer.

Nevertheless, looking at the relationship between attrition and enlistment bonuses through much of the 1990s, we see that "all other factors" were not holding constant. If we simply compare attrition rates of accessions during this period, we observe that those who received larger EBs were *more* likely to attrite. This positive correlation was apparently the result of sharply improving conditions in private-sector employment; while the services were increasing enlistment

¹ I am grateful to Michael Hansen for help in estimating the hazard models for this paper, to Ted Jaditz for support in working out the econometrics in appendix B, and to David Reese for constructing the data sets used in this analysis.

² See the bibliography for a list of recent studies on enlistment bonuses.

bonuses in an effort to offset rising private wages and declining unemployment, meeting enlistment goals became increasingly difficult, and attrition—particularly among the most skilled accessions—greatly increased.

A second factor driving the positive relationship between attrition and the value of enlistment bonuses was an apparent policy change introduced in the mid-1990s that sharply increased the number of, and value of, enlistment bonuses offered to off-peak accessions (accessions who enter the service between October and May). These are typically people who have worked for some time after leaving high school, and who often view the military as a second-choice career. They tend to have fewer of the characteristics necessary for success in the service than accessions who ship directly from high school.

Objectives and approach

In this analysis, we attempt to separate the various elements of the relationship between attrition and EBs, and to focus on how the value of the enlistment bonus affects the likelihood of attrition. In attempting to sort out such effects, economists typically use estimation techniques that control for sample selection and omitted variables. Unfortunately, these methods are not applicable given the data available to this analysis, and we must use an alternative approach. The technique that we employ starts with estimating the relationship between attrition and various observable explanatory variables—including the value of the EB—without controlling for sample selection or missing variables. The results from these estimations are known to be biased. However, we impose some rather straightforward restrictions on our estimations, and these permit us to predict that the sign of this bias is positive. This means that, if we identify a negative relationship between EBs and losses, we can be certain that bonuses reduce attrition *by at least* this much.

Results

Our analysis uses data on Navy enlisted personnel who shipped from the beginning of FY93 to the end of FY97, and we follow the attrition behavior of these cohorts through the end of FY99. Of these

accessions, enlistment bonuses were offered only to those who shipped with 5 or 6 years of obligation (5YOs or 6YOs).

When we analyze our data at the most aggregated level (pooling all years of entry, all programs of entry, and both 5YOs and 6YOs), our regressions show either a *positive* relationship between EBs and attrition, or no statistically significant relationship between these variables (depending on the functional form that is employed in the analysis). However, upon detailed examination of our data, we find that, for a substantial part of our study period, there are compelling reasons why we should expect, *a priori*, that regressions should *not* yield a negative and significant relationship between the size of the EB and attrition:

- Over some parts of the survey period, and for some programs of entry, there was little variation in EBs, and this would have acted against our identifying a significant relationship between EBs and attrition.
- Throughout the survey period, the sharp shift toward offering EBs to off-peak accessions meant that the Service was directing increasing portions of the EB budget to personnel with a higher propensity to attrite.
- In addition, over the study period, there were increasingly rapid changes in both the characteristics of recruits and economic conditions. As a result, changes in EBs were driven more and more by the Service's attempts to keep up with private-sector wages. (One should expect to observe a negative correlation between enlistment bonuses and attrition only when economic conditions and recruit characteristics are stable—in other words, we should expect that the most reliable estimates would come from analyses of those who accessed early in the study period.)

Appendix A shows the results of 11 regressions, representing a combination of entry years and programs of entry. Of these 11 regressions, the relationship between EBs and attrition is negative and significant only twice. The importance, or credibility, of these two cases is enhanced by the fact that they occur in analyses of personnel who accessed early in the study period. Even so, when we weigh all of

our empirical results, it must be said that we find only limited evidence that increasing EBs reduces attrition.³

While we believe that these statistical results are not substantial enough to guide policy, we suggest that they are sufficiently compelling to justify the Navy pursuing experiments on this issue. That is, on the basis of this analysis, we suggest that the Navy should fund experiments to explore the effects of different enlistment incentives on retention.

Implications

Enlistment bonuses have typically been viewed as being useful in recruiting—routing personnel to a specific branch of Service, directing recruits into hard-to-fill ratings, extending sailors' obligated service, and increasing accessions during off-peak months. The findings of the current study offer some, limited support to the idea that enlistment bonuses may have a secondary effect of decreasing attrition.

The findings of the current study, however, should be viewed as preliminary. For some cohorts of accessions, we have been *partially* successful in disentangling the attrition effects of EBs from the attrition effects of other factors—such as changes in the characteristics of those who have been offered bonuses. A more precise analysis of how EBs affect attrition would require an experiment in which a sample of accessions has a portion of their enlistment bonus randomly assigned. This type of experiment was undertaken 20 years ago to assess the effect of EBs on the *enlistment* behavior of recruits. Given the rapid expansion in the use of EBs over the last half decade, a similar experiment might now be in order to assess the impact of the full set of enlistment incentives on attrition and other career decisions.

³ Although we do feel that we have found limited evidence that EBs lower attrition, we are not confident enough in our results to extrapolate from our regression results and to present exact estimates of these effects.

Background and policy issues

The historical use of enlistment bonuses

The enlistment bonus (EB) is one of the more flexible tools that the Navy has to meet its recruiting goals. Most elements of military pay (e.g., basic pay and housing allowances) are uniform across all new recruits, but the EB can be offered at different levels to various classes of accessions.⁴ The Navy has traditionally employed these bonuses to meet several objectives:

- To attract more skilled accessions into the military
- To encourage specific types of recruits to enlist in the Navy (rather than into some other service)
- To route personnel into certain hard-to-fill ratings
- To encourage recruits into longer terms of service
- To increase shipping during winter and spring months.

A recent, rapid rise in the use of enlistment bonuses has resulted in keen interest in determining the effectiveness of the EB as a tool in military personnel management. Over the 1990s, it became increasingly difficult for the Navy to meet its recruiting goals, and the service expanded its use of EBs from about \$15 million in FY97 to almost \$100 million in FY02. This represents large increases in both the average EB paid to recruits and the proportion of accessions who are promised a bonus.

⁴ Other recruiting tools tailored to different classes of recruits include the Navy College Fund and the Navy College Loan Repayment Plan.

How enlistment bonuses can affect the level of attrition

Although the principal motivation for using EBs has always been to influence the *enlistment* choices made by those considering service in the military, these bonuses likely affect attrition as well. It is useful to think of the relationship between EBs and attrition as being composed of three elements.

First, holding all else constant, if we offer an individual more pay (a larger bonus), he or she is likely to remain in the service for a longer period. Although recruits weigh a number of factors when making their career decisions (the desire to serve their country, acquiring skills on the job, etc.), both recruiting and retention are sensitive to the level of military pay.⁵

A second linkage between enlistment bonuses and attrition results from using EBs to offset rising civilian wages. In this context, rising bonuses can signal a tightening labor market and may presage higher attrition. If larger bonuses offered by the services only *partially* compensate for rising wages in private employment, we may observe higher levels of attrition and reduced levels of reenlistment despite larger bonuses.

Finally, enlistment bonuses may also affect attrition by changing the type of person who joins the service. A person's willingness to enlist is affected by both military pay, relative to civilian employment, and the person's taste for military life. Holding all else constant, the services could attract people who are less committed to military service by offering them larger bonuses. If, as seems likely, those with less taste for the military are more likely to attrite, larger bonuses may induce higher losses from the service.⁶

⁵ See Goldberg (2001) on wage elasticities among military personnel.

⁶ This would be consistent with several studies that have observed that military personnel who receive reenlistment bonuses at the first decision point are *less* likely to reenlist at the second decision point, compared with persons who did not receive a bonus at the first decision point. For example, see Warner and Simon (1979), Rodney et al. (1980), and Kohler (1988).

Forces affecting the level of attrition during the study period

Significant changes occurred in the recruiting environment during the study period (FY93 to FY97). We observed large increases in bonuses, significant changes in the characteristics of those promised enlistment bonuses, a great reduction in private-sector unemployment, and sharp increases in attrition among many of the ratings that receive EBs. During this time, the average bonus offered 5YO_s increased from \$1,249 to \$3,564, and the average offered 6YO_s in the nuclear field⁷ rose from \$3,225 to \$6,934.⁸ (As table 1 shows, among cohorts who accessed from FY93 to FY97, only select 5YO_s and 6YO_s were promised bonuses.) The growth in the value of EBs appears to have been caused by a significant tightening in the civilian labor market: in our study period, the unemployment rate declined from 6.9 percent to 5.1 percent.⁹ The heating up of the labor market, in turn, appears to have affected attrition, particularly for 6YO_s entering the nuclear field: among nuclear 6YO_s promised an EB, attrition over the first 24 months of service increased from 19 percent in FY93 to 27 percent in FY97.

During this period, several notable changes occurred in the personal characteristics of those promised enlistment bonuses (see tables 2 through 4). Perhaps the most striking of these was a shift toward giving more bonuses to off-peak accessions. In FY93, 59 percent of the 5YO_s receiving bonuses accessed during the peak season (June to September), but by FY97 this figure had declined to 30 percent. A similar trend is evident among the 6YO_s accessing into the nuclear field.

Accessions who ship during off-peak periods are less likely to be coming to the service directly from high school. We see this reflected in the fact that, among 5YO_s promised a bonus, the average age at accession rose from 19.6 in FY93 to 20.2 in FY97. We also observed a

⁷ The nuclear field is defined by program of entry at time of accession.

⁸ These are average enlistment bonuses among those promised an EB.

⁹ This unemployment rate is the average home-state unemployment among all 6YO_s in the nuclear field.

Table 1. Attrition rates by years of obligation, fiscal year, and promise of enlistment bonus among study sample

Years of obligation (YO)	Losses		Number of observations
	12-month	24-month	
3YOs			
1993	0.173	0.247	543
1994	0.215	0.312	15,291
1995	0.211	0.300	11,673
1996	0.193	0.282	8,137
1997	0.215	0.308	4,911
4YOs			
1993	0.200	0.290	33,635
1994	0.195	0.285	22,293
1995	0.216	0.315	20,675
1996	0.214	0.314	22,024
1997	0.226	0.319	27,401
5YOs			
1993	0.208	0.278	1,866
1994	0.167	0.236	5,676
1995	0.179	0.260	6,159
1996	0.179	0.266	5,824
1997	0.180	0.258	5,293
5YOs promised EB			
1993	0.235	0.313	793
1994	0.231	0.322	1,421
1995	0.211	0.314	1,920
1996	0.222	0.351	1,789
1997	0.201	0.311	1,002
6YOs			
1993	0.143	0.212	4,830
1994	0.143	0.224	4,920
1995	0.165	0.269	4,375
1996	0.182	0.285	4,176
1997	0.208	0.307	4,630
6YOs promised EB			
1993	0.122	0.195	1,806
1994	0.127	0.206	2,189
1995	0.152	0.259	2,519
1996	0.180	0.286	3,675
1997	0.208	0.307	4,288

rise in the proportion who are married (among 5YO_s, this increased from 3.9 percent to 6.2 percent).

There may be other differences between peak and off-peak acces-sions that are important but unobservable. Those who ship in off-peak months are more likely to have worked in the private sector di-rectly after high school. This suggests that the service may not have been their first choice of career, and they may be less motivated to serve in the military than those who ship during the peak season. Moreover, off-peak shippers who are leaving jobs may be doing so be-cause they have had difficulty with their employers, and this may in-dicate that they lack the characteristics necessary for success in the working world.

How enlistment bonuses can affect the *timing* of attrition

Paying enlistment bonuses may also affect the timing of attrition. The importance of this effect, however, depends on how service per-sonnel perceive the military's ability (and willingness) to recoup bo-nuses from those who accept an EB and who subsequently attrite.¹⁰ If recruits anticipate a bonus at a particular time in their careers, and if they believe that the bonus carries no effective obligation for further service, the likelihood of attrition will decline as the time of the bo-nus payment approaches. Under such circumstances, attrition would likely rise shortly after the payment of the bonus because we would see the departure of those who had postponed leaving the service in order to acquire the bonus.

¹⁰ The services are authorized to recoup EBs from those who accept a bonus but fail to complete the term of service for which the incentive was paid. Nevertheless, anecdotal evidence suggests that the services may find it uneconomical to try to recoup funds from these persons: many who at-trite from the service after receiving an EB have already spent their bonus and are likely to have few assets. For information on recoupment, see *DoD Financial Management Regulation*, Vol. 7A, Chapter 9.

Table 2. Variable means by year among the sample of 5YOs who were promised an enlistment bonus

	1993	1994	1995	1996	1997
Number of accessions in sample	793	1,421	1,920	1,789	1,002
Proportion of sample attriting in 12 months	0.23	0.23	0.21	0.22	0.20
Average EB promised among those in sample (dollars)	1,249	1,348	1,317	2,347	3,564
Average age at accession among those in sample (years)	19.59	19.76	20.05	20.21	20.22
Proportion of those in sample who are in Tier 1*	0.96	0.95	0.89	0.81	0.83
Average AFQT among those in sample	57.68	60.74	62.09	67.54	72.74
Proportion of those in sample who are male	0.92	0.83	0.80	0.92	0.89
Proportion of those in sample who are white	0.70	0.75	0.73	0.73	0.77
Proportion of those in sample who are married	0.039	0.049	0.054	0.052	0.062
Proportion of those in sample accessing in peak**	0.59	0.46	0.33	0.30	0.30
Average unemployment in home state at accession	6.75	6.29	5.60	5.60	5.05

* Tier 1 accessions have at least a high school degree.

** The peak season for accessions is June through September.

Table 3. Variable means by year among the sample of non-nuclear 6YOIs who were promised an enlistment bonus

	1995	1996	1997
Number of accessions in sample	579	2,076	2,544
Proportion of sample attriting in 24 months	0.30	0.30	0.33
Average EB promised among those in sample (dollars)	2,478	4,607	5,154
Average age at accession among those in sample (years)	20.34	20.13	20.29
Proportion of those in sample who are in Tier 1*	0.89	0.89	0.87
Average AFQT among those in sample	79.82	80.23	79.54
Proportion of those in sample who are male	0.88	0.91	0.88
Proportion of those in sample who are white	0.79	0.80	0.78
Proportion of those in sample who are married	0.07	0.06	0.07
Proportion of those in sample accessing in peak**	0.64	0.39	0.33
Average unemployment in home state at accession	5.64	5.48	5.05

* Tier 1 accessions have at least a high school degree.

** The peak season for accessions is June through September.

Table 4. Variable means by year among the sample of nuclear 6YOs who were promised an enlistment bonus

<u>Year</u>	1993	1994	1995	1996	1997
Number of accessions in sample	1,806	2,189	1,940	1,599	1,744
Proportion of sample attriting in 24 months	0.19	0.21	0.25	0.27	0.27
Average EB promised among those in sample (dollars)	3225	3228	3611	5666	6934
Average age at accession among those in sample (years)	19.20	19.13	19.33	19.76	19.61
Proportion of those in sample who are in Tier 1	1.00	1.00	1.00	1.00	1.00
Average AFQT among those in sample	89.91	89.65	89.91	90.04	89.48
Proportion of those in sample who are male	1.00	0.98	0.92	0.91	0.93
Proportion of those in sample who are white	0.88	0.89	0.85	0.82	0.81
Proportion of those in sample who are married	0.043	0.032	0.039	0.049	0.050
Proportion of those in sample accessing in peak	0.44	0.34	0.27	0.26	0.25
Average unemployment in home state at accession	6.88	6.23	5.57	5.51	5.07

* Tier 1 accessions have at least a high school degree.

** The peak season for accessions is June through September.

Analytical approach

Our objective in this study is to identify how enlistment bonuses affect attrition, holding constant the quality of recruits, relative pay in military and civilian employment, and all other relevant variables. We wish to consider the effect of enlistment bonuses on both the level and timing of attrition; however, no single set of analytical tools permits us to evaluate both these effects. For this reason, we separate our empirical work into two parts.

In the first section, we examine how the size of the enlistment bonus promised to a recruit affects the likelihood of achieving critical milestones in the first term of service. Among 5YO_s this is the first anniversary in the service; among 6YO_s it is the second anniversary.¹¹

This analysis is based on a binary regression model. In this part of our work, we must address two empirical complexities:

- Sample selection bias. Those in our sample who are promised enlistment bonuses are likely to differ in important respects from the average Navy enlisted person (e.g., they are likely to be brighter and to have greater opportunities in civilian employment). Failure to control for these differences, or to at least recognize their effect on our analysis, could produce spurious predictions about how EBs affect attrition if the Navy changes the types of individuals to whom they offer EBs.
- Omitted variables bias. There are likely to be factors that have important effects on attrition but that we are unable to measure (e.g., opportunities in civilian employment). If we ignore the impact of these missing variables, we might incorrectly attribute their effects to enlistment bonuses.

Later in this report, we discuss these potential biases in greater detail and describe our approaches to dealing with them.

¹¹ These anniversaries roughly correspond to the time when accessions complete their training.

From the first part of our empirical analysis, we are able to assess whether offering larger enlistment bonuses has lengthened first-term service *among the recruits in our sample who were promised an EB.*

In the second part of our analysis, we explore the effect of EBs on the timing of attrition. Using a discrete-time hazard model, we predict attrition behavior as a function of the size of the enlistment bonus. We then employ this prediction to trace out how the temporal pattern of attrition would likely change were the military to increase its offers of EBs.

Finally, we consider how the services might gather better data on enlistment bonuses to permit more complete analyses of the effects of EBs on the career choices of military personnel.

EBs and the level of attrition

In this section, we assess the attrition effect of enlistment bonuses using a simple, binary measure of loss—whether an accession remains in the service to the 1-year anniversary (in the case of 5YOIs) or to the 2-year anniversary (in the case of 6YOIs). These milestones correspond roughly to the time when accessions complete their training and reach their first full duty billet. These are especially useful milestones for measuring loss because training is the time of the greatest likelihood of first-term attrition.

Data

Our study is based on data for all enlisted accessions who entered the Navy between the beginning of FY93 and the end of FY97. Our sample begins in FY93 because, before that time, Navy data do not have reliable information on the date a recruit entered the delayed entry program (DEP). This is a serious limitation because the size of the enlistment bonus that is promised to an accession is a function of (1) the rating he is promised, (2) the date of entry to DEP, and (3) the date on which he or she ships to bootcamp. Without a reliable DEP date, we would be unable to verify the size of the EB promised those who accessed before FY93. Our sample ends with those who accessed at the close of FY97 because we wished to have at least 2 years of attrition behavior for everyone in our sample.

During FY93 through FY97, only enlisted accessions who entered the Navy with 5- or 6-year obligations were offered EBs. In our analysis, we partition our sample by years of obligation because it seems likely that the impact of EBs on 5YOIs would be different from the impact on 6YOIs: for one thing, 5YOIs typically have to wait only half as long as 6YOIs to receive their bonuses.

The sample selection issue

Our approach to evaluating the effect of enlistment bonuses on attrition is to look at the career decisions of those who have been promised EBs. However, Navy recruits who were offered bonuses in FY93 to FY97 differed from the average enlisted personnel in important respects. Because of these differences, one must take care in making policy recommendations for the entire Navy based on the behavior of those in our sample of recruits promised an EB.

Over the period for which we have data, the largest EBs were offered to those who were signing on to serve in the most technically demanding ratings, those who agreed to serve for the longest terms, and those who entered the service when the unemployment rate was at its lowest level. In short, they were brighter accessions, who had stronger than average opportunities in civilian employment, and who were facing a long obligation in the service.

One could make a strong argument that these "high quality" accessions are less sensitive than the average recruit to monetary incentives. This, however, is an empirical question that needs to be tested against hard data. An ideal study of the attrition effects of enlistment bonuses would *jointly* estimate (a) the determinants of whether a person has been offered a bonus, and (b) the factors affecting whether a person remained in the service, controlling for his or her selection into the group promised an EB. Such an approach would enable us to predict how EBs, and other explanatory variables, might affect attrition *among types of recruits who have not previously been offered EBs*.

Unfortunately, this ideal approach does not seem possible in the context of the current study.¹² Our analysis, therefore, is limited to assessing whether offering larger enlistment bonuses acts to extend the average length of first-term service *among the type of recruits who have typically entered the Navy with the promise of an EB*.

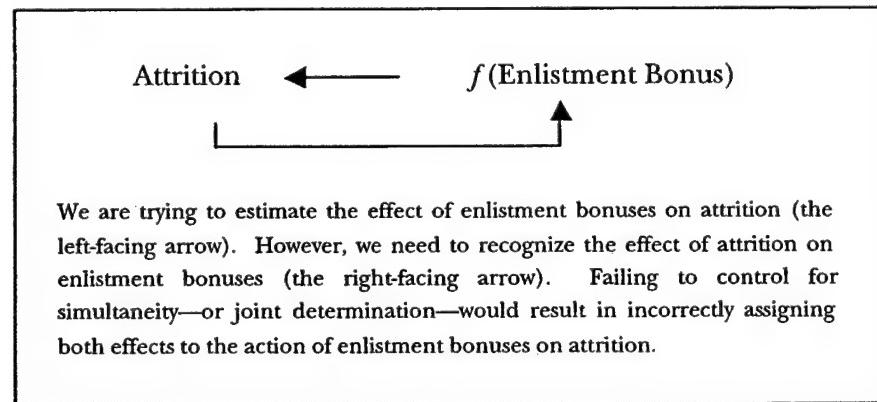
¹² In order to jointly estimate these two events, we need variables that explain selection into the group offered bonuses but that have no additional impact on attrition.

Misspecification

Simultaneity

When we estimate the effect of enlistment bonuses on attrition, we need to recognize that EBs and losses from the service can be linked for more than one reason. In the literature on bonuses and retention, this issue has typically been described in terms of simultaneity bias [see Goldberg (2001)]. When we examine group (cohort) data, retention and the value of enlistment bonuses may be simultaneously determined—that is, each may influence the other: larger enlistment bonuses may dissuade personnel from attriting, but higher levels of attrition (together with weakness in accessions) may induce policymakers to increase the value of enlistment bonuses. Were we to ignore the potential for this sort of joint determination of attrition and enlistment bonuses, we could produce a biased estimate of how enlistment bonuses affect attrition. (With individual-level data, this problem with simultaneity is mitigated, although Goldberg (2001) discusses scenarios in which it still might occur.)

Figure 1. Simultaneous determination of attrition and EBs



Omitted variables

When we use individual data to analyze attrition (as in the current study), simultaneity bias is typically not a concern. However, a closely related issue—omitted variables bias—can hinder our analysis.¹³ When we fail to include important determinants that are correlated with bonuses, we may attribute to EBs the effects of these missing variables. This issue is especially worrisome in the context of the current study because we are unable to include among our explanatory variables two factors that are likely to have critical influences on attrition: the quality of military accessions (whether recruits have the characteristics necessary for success in the service), and changes in lifetime earnings in both military and civilian employment.¹⁴

Not only are we missing key variables, but, over our study period, there is likely to be a strong correlation between these omitted variables and enlistment bonuses. The value of EBs was increasing in every year of this period. Moreover, as we have previously observed, employment opportunities in the civilian sector were steadily improving and, we suspect, the quality of accessions was steadily deteriorating.

We address the problem of unobserved variables in two ways. First, in addition to estimating the relationships between attrition and bonuses (and our other explanatory variables) for the entire study period, we estimate these relationships for each year. Undertaking estimates on a year-by-year basis reduces the potential for bias arising from the omission of variables that have significant variation across time periods, but less variation within time periods.

¹³ In appendix B, we demonstrate that simultaneity bias can be thought of as part of the larger issue of omitted variables bias.

¹⁴ The explanatory variables used in this study include the unemployment rate in a recruit's home state, and this provides some indication of *current* labor conditions. A recruit's labor supply decision, however, is likely to be based on perceptions of expected *lifetime* earnings in both private and military employment. Economists have estimated potential military and civilian lifetime earnings for older service personnel [see Hansen et al. (2001) for a discussion of this literature], but it seems impractical to make these types of predictions for the young personnel in our sample because they have yet to achieve a rating or to show their aptitude for work.

Our second approach to addressing the omitted variables issue is to “sign the bias.” In appendix B of this analysis, we demonstrate that, with a few weak assumptions, we can anticipate that omitted variables will result in a positive bias in our estimates of how enlistment bonuses affect attrition. This is an especially useful finding because it allows us to interpret our estimation results as providing a *bound* on the true relationship (i.e., if our estimates indicate that a \$1 increase in the promised enlistment bonus results in an x -percent reduction in attrition, we know that a \$1 increase in bonus will actually yield a drop in attrition of *at least* this much).

The intuition behind the positive sign on the bias is that our data include cases in which higher attrition is associated with higher EBs:

- Better private-sector employment conditions result in higher attrition and at the same time may induce the Navy to offer higher EBs.
- Giving more EBs to off-peak accessions means that a higher attrition group is getting higher EBs.

Since we are not able to fully control for these factors, the estimated coefficient on how EBs affect attrition will be biased upward.

Definitions of variables used in the binary analysis

The dependent variable

Attriting before completion of training. The dependent variable takes on a value of 1 if an accession attrites before achieving a particular milestone in his service career. For 5YOs, this is 1 year in the service, and for 6YOs it is the 2-year mark. These anniversaries are close to the times when accessions are scheduled to complete their training, to be paid their enlistment bonus, and to reach their first active duty billet. They also mark the end of the period in which accessions are most likely to attrite from the service.

The independent variables

Amount of enlistment bonus. This is the amount of enlistment bonus that the accession was promised for successfully completing training and attaining the rating for which he or she contracted. CNRC staff provided these data as a "look-up table" that defined the amount of bonus in terms of (1) promised rating, (2) date on which the recruit entered DEP, and (3) date on which the person shipped to boot camp. These data were validated by checking the amount of bonus indicated in the look-up table with the amount that was paid out to those who attained the rating for which they initially contracted.

AFQT. The Armed Forces Qualification Test (AFQT) is the preliminary exam given to all recruits to assess the likelihood of their being able to complete training. The test includes measures of arithmetic reasoning, knowledge of math, reading comprehension, and word knowledge. The variable is reported as a percentile. For further information on this measure, see Kilburn et al. (1998).

Age. This is the age of the recruit at the time of accession.

Caucasian. This is a binary variable that equals 1 if the recruit identifies himself or herself as Caucasian, and is otherwise equal to 0.

Gender. This is a binary variable that equals 1 if the accession is male. Note that this variable is omitted from the regression for 5YO's in 1993 because virtually all accessions were male.

Marital status. This binary variable equals 1 if the accession indicates that he or she is married, and is otherwise equal to 0.

Peak-season accession. This is a binary variable that equals 1 if a recruit ships to boot camp during June, July, August, or September. Because of high school graduation schedules, about half of accessions ship during these months. We recognize that the time of shipping may be endogenous because recruits may be induced to access during off-peak months by the offer of larger enlistment bonuses. For this reason, we have tested our results to the inclusion of the peak-season binary variable and have found that incorporating this measure has no effect on our estimates of how EBs affect attrition.

Binary variables for FY. These variables equal 1 if the person accessed in the indicated fiscal year; otherwise, they equal 0.

Interpreting biased results

The binary estimates can provide information in several ways. First, they can inform us if enlistment bonuses increase the probability of reaching a particular milestone. As we discussed earlier (and will discuss in appendix B), our estimates of the effect of bonuses on attrition are likely to be biased. However, given a few weak assumptions (described in appendix B), we can predict that the sign of this bias is positive. This means that, if we identify a negative relationship between EBs and losses, we can be certain that bonuses reduce attrition by *at least* this much.

Moreover, we can also take useful information from estimation results that show a *positive* relationship between EBs and attrition. For example, if we observe a positive relationship between EBs and losses over several years, this implies that attrition may be influenced by either of the two types of estimation bias that are of greatest concern to this analysis: either bonuses are failing to keep pace with (compensate for) wage increases in the civilian sector, or larger bonuses are being paid to “lower quality” accessions (lower quality in the sense that they are less likely to remain in the service).

Our estimations may allow us to determine which unobserved effect dominates in driving such results. For example, if we find a positive relationship between the value of EBs and attrition for specific cohorts (groups that access within a short time of one another), this is unlikely to be driven by changes in economic conditions over time; it is more likely to be driven by differences in the quality of accessions. This is because, when we look at data that span only a short period of time, whatever variation in behavior we observe can only be caused by variation in *contemporaneous* variables.

The estimation results

Cohorts for which EBs lower attrition

Tables 6 through 8 in appendix A show the results of regressing the binary attrition variable against our explanatory measures. The data come from 5 years and 3 programs of entry (5YO, 6YO nuclear, and 6YO non-nuclear), which with two missing cells gives us a total of 13 cells. From this, we report the results of 11 regressions, which resulted from combining two sets of year cells for the 5YOs.

Out of these 11 regressions, we get negative and significant estimates twice. Given our data and estimation techniques, then, it first must be said that we have only scant evidence that increasing EBs reduces attrition. The results may be somewhat better than this indicates, though, because of the 11 regressions, there are several for which we should not expect, *a priori*, to see a statistically significant negative relationship between the size of EB and attrition. In the case of some of the 11 regressions, there is insufficient variation in EBs to permit a credible statistical analysis. In other cases, particularly those late in the study period, the descriptive statistics show that both the characteristics of recruits and economic conditions are changing very quickly. *A priori*, one should expect to find a statistically significant negative relationship between EBs and attrition only in years in which recruit characteristics and economic conditions were relatively stable—the years early in the study period. This is, in fact, where we find the statistically significant negative relationships.

Furthermore, we have shown that our estimates have a positive bias, because strengthening civilian employment conditions and increasing offers to off-peak season accessions tend to increase both EBs and attrition. For this reason, we find it somewhat remarkable that we found any evidence of EBs reducing attrition, which tends to lend more credence to these results.

The most striking findings are those for 5YOs during FY93 to FY94, when larger enlistment bonuses are associated with a sizable reduction in the likelihood of attrition. We also observe a negative and significant relationship between EBs and attrition among non-nuclear 6YOs who accessed in FY95.

Further extensions

For both the 5YO_s and the non-nuclear 6YO_s, the negative and significant coefficients for the enlistment bonus variable are highly robust to the inclusion and exclusion of other explanatory variables. In particular, omitting the variable "accessed during peak season" results in little change in the estimated coefficient for "value of enlistment bonus."

In appendix B, we show that, when we regress the binary attrition measure against a small number of explanatory variables (including "the size of the enlistment bonus"), we can "sign the omitted variables bias" for this estimation model.¹⁵ When we undertake such regressions for both the 5YO_s and the non-nuclear 6YO_s, we find negative and significant coefficients on "the value of the enlistment bonus" that are very similar to those shown in the regressions with the full set of observable explanatory variables.

Attrition effects of EBs among peak-season accessions

For another set of regressions, we partitioned our sample by season of accession¹⁶ and estimated attrition against our set of explanatory variables (these regressions are shown in table 9 of appendix A). The results from these estimations suggest that attrition is sensitive to the value of the enlistment bonus only among those 5YO_s who accessed in the *peak seasons* of FY93 and FY94 (attrition is unaffected by EBs among those who shipped off-peak). Among non-nuclear 6YO_s who accessed in FY95, the attrition effect of EBs is also greater among

¹⁵ It would be desirable to "sign the omitted variable bias" in regressions that include all of our observable explanatory variables (race, AFQT, etc.). Note, however, that the attrition effects of many of the observable variables (e.g., marital status and age) are likely to be small compared with those of omitted variables, such as the quality of recruits and the wage differential between private and public employment.

¹⁶ This approach carries a caveat. If accessions choose ship dates to maximize EBs, we would be partitioning our sample on an endogenous variable: this could bias our estimates. Although these results point to an interesting relationship between EBs and attrition, because of the potential bias, we do not report these estimates among the conclusions of this paper.

peak-season accessions, although the difference is less pronounced than among 5YO_s (the results for 6YO_s are not included among our tables).

One needs to exercise caution in making distinctions about how EBs affect attrition among recruits who access at different times of year. The fact that we cannot *identify* a strong attrition effect for EBs promised to off-peak accessions may not indicate that such an effect does not exist. Rather, our inability to identify such a relationship may simply reflect greater heterogeneity among off-peak accessions than among peak-season accessions (who are typically coming straight from high school): the service may be paying more to attract off-peak accessions who are at greater risk for attrition, either because they have greater opportunities in private employment or because they have less intrinsic interest in serving in the military. Our regressions may be failing to identify reductions in attrition that result from paying EBs to these off-peak accessions simply because we cannot control for the unobservable characteristics that make some recruits higher risks for attrition.

Cohorts for which there is no evidence that EBs reduce attrition

Because of the positive bias, in a sense it is surprising that we find any evidence among our sample cohorts that enlistment bonuses reduce attrition. To assess the effects of EBs on losses, one would like to examine a sample that has significant variation in the size of enlistment bonuses being promised to recruits but little variation in economic conditions, the policies governing the administration of enlistment bonuses, or the characteristics of recruits.

Our study period, however, was marked by continuous improvement in private employment conditions and a notable deterioration in the recruiting environment. One might expect that whatever increases in bonuses occurred during these years would have been driven by the service's need to offset improved private sector wages. If this were the case, we would expect to observe a *positive* correlation between the size of the enlistment bonus and the probability of attrition.

Over our study period, there were also aspects of Navy policies on enlistment bonuses that make it unlikely that we would observe a

negative correlation between attrition and the value of EBs promised to recruits. We have already discussed how the sharp shift toward offering EBs to off-peak accessions is likely to have changed the characteristics of the average bonus recipient, and to have resulted in greater attrition among those offered larger bonuses. In addition, over the study period, there was often far less variation in the value of enlistment bonuses for peak-season than off-peak accessions (see table 5). For example, among those who accessed into nuclear ratings during the peak season of FY97, the bonus mean was \$3,300 and the standard deviation was only \$113. By comparison, among nuclear personnel who accessed off peak that year, the mean bonus was \$8,200 and the standard deviation was \$2,000. The small variation in EBs for peak-season accessions makes it unlikely that our estimation techniques would reveal *any* relationship between bonuses and attrition for peak-season shippers. As we have previously pointed out, however, it is among peak-season accessions that we would be most likely to find a negative relationship between bonuses and attrition.

Table 5. Enlistment bonus statistics by years of obligation, year of accession, and season of accession

Sample	Peak (June to Sep.)			Off-peak (Oct. to May)		
	Mean EB	S.D. of EB	No. of obs.	Mean EB	S.D. of EB	No. of obs.
5YO_s promised an EB						
FY93	1,173	670	469	1,361	978	324
FY94	1,092	511	651	1,565	828	770
FY95	1,320	373	624	1,317	631	1,296
FY96	2,461	1,315	528	2,300	2,085	1,261
FY97	2,688	1,446	305	3,948	2,495	697
Nonnuclear 6YO_s promised an EB						
FY95	2,790	1,241	370	1,923	2,078	209
FY96	3,048	707	809	5,602	2,581	1,267
FY97	3,049	322	836	6,184	1,931	1,708
Nuclear 6YO_s promised an EB						
FY93	1,999	28	803	4,207	897	1,003
FY94	2,002	73	747	3,863	985	1,442
FY95	2,016	198	531	4,212	1,365	1,409
FY96	3,191	539	423	6,557	2,112	1,176
FY97	3,308	113	441	8,161	2,025	1,303

Several points are notable about the regression results that show a positive and significant relationship between attrition and enlistment bonuses. First, the fact that we observe this relationship in data for personnel who access within a few months of each other (the data are essentially cross-sectional) suggests that those who are being offered larger enlistment bonuses have a higher intrinsic likelihood of attrition than those being offered smaller bonuses. They may be people with greater opportunities in the civilian labor market, those with less taste for military service, or those entering ratings with more difficult training pipelines.

The positive correlation between attrition and the value of enlistment bonuses does not necessarily indicate a policy failure. It may be rational for the services to offer larger EBs to people who have a greater propensity to attrite if the alternative is to recruit fewer people.

We should also point out that the positive coefficient on EB in the attrition equation does not necessarily imply that raising enlistment bonuses would raise losses. The fact that larger bonuses are offered to persons who are at greater risk for attrition does not tell us how a specific recruit would respond to the offer of a larger bonus: it is possible that, even among those who are at the highest risk of attrition, increasing the bonus that they are offered could lower the probability of their attrition.

EBs and the *timing* of attrition

As we have previously discussed, the size of the enlistment bonuses may affect both the level and timing of attrition. In this section, we examine the effect of EBs on the timing of losses among those cohorts for which our probit regressions indicated a negative and significant relationship between the size of the EB and the probability of attrition (these include 5YO_s who accessed in FY93 and FY94, and nonnuclear 6YO_s who accessed in FY95). Notice that we only look at the 2 out of 11 cohorts for which we found negative, significant results in the previous section.

For this analysis, we use a discrete-time hazard model. The principal benefit of a hazard model is that it permits us to represent, in a single estimation, the temporal pattern of attrition across the entire first term. Using the results of the discrete-time hazard model, we predict attrition behavior as a function of the size of the enlistment bonus, and employ this prediction to trace out how losses would likely change were the military to sharply increase its offers of EBs.

Using a discrete-time model—rather than a continuous-time model—permits the use of independent variables that change value over time; this is central to our study because accessions who fail their training lose their eligibility for an enlistment bonus. Appendix C of this paper describes the technical characteristics of the discrete-time hazard model.¹⁷

¹⁷ For general material on hazard models, see Cox and Oates (1984).

Definitions of variables used in the hazard analysis

The dependent variable—the month of attrition

In our implementation of the hazard model procedure, each discrete month of an accession's career is treated as a separate observation. For each observation, the dependent variable assumes a value of 1 if the accession attrites from the service during that month and takes on a value of 0 if the accession has not attrited. No observations are generated for the time after a person either attrites or successfully completes his or her first term of service. A person is defined as successfully completing service if, at the time of departure, he or she is within 3 months of completing the *current* soft EAOS for the first term.¹⁸

The independent variables

Time variables. For several key events in the first term of service, we include a binary variable indicating that the event has occurred, a variable indicating the number of months since the event, and a variable for the number of months squared. These key events include the following: arrival at boot camp, beginning A-School training, completion of A-School training, payment of the EB, and deployment to first full duty billet.

The value of the promised enlisted bonus. The value of the enlistment bonus is derived from the same data as the EB measure used in the binary analysis of attrition, but differs from that regressor in important respects. Accessions who fail the A-School training of the rating for which they were promised an EB are no longer eligible for an enlistment bonus. As a result, the value of the promised EB variable goes to 0 in the month an accession flunks out of the initial A-School pipeline. Though it would have been desirable to make a similar

¹⁸ The soft EAOS is the last day of the member's total active duty obligation. It includes any executed agreements to extend enlistment or active duty whether or not they have become operative.

change to the enlistment bonus for those 6YOs who fail their G School requirements, we were unable to do this.¹⁹

The value of the enlistment bonus is entered into our regressions in two regressors: the dollar value of the bonus before payment of the EB, and the value of the bonus after payment of the EB. We separate the bonus measure in this way to allow for the possibility that larger EBs might induce a greater “bounce” in attrition after the payment of the bonus (a larger EB might induce more people to postpone departure from the service until after the payment of the bonus).

Other independent variables. These include measures for age, race, marital status, AFQT, educational tier, and current unemployment. These are defined in the same way as in our binary analysis of attrition and are described earlier.

Results

The results in this section should be considered as illustrative of possible outcomes if EBs were increased and not as actual predictions. This is because, first, they are based only on the 2 out of 11 cohorts in which we could find negative and significant results. Second, the examples all involve increasing the EB by large amounts, rather than the marginal increases that should be used.²⁰

¹⁹ The process of identifying the training pipelines associated with various ratings is complex, as the training requirements of ratings change and courses with different numbers can be substituted for each other. We were able to identify the A-level courses for those ratings for which accessions were promised EBs. We did this by looking at the courses taken by persons who successfully attained the rating that they were promised. However, this approach could not be used to identify the GSchool requirements of each rating because there were far too many courses taken by accessions who “made their rating.”

²⁰ This type of statistical approach can only show how small changes in enlistment bonuses affect overall attrition (e.g., how overall attrition would change if the total budget for bonuses were increased by \$1,000). However, we have used a *doubling* of enlistment bonuses to better illustrate how changing bonuses affects the timing of attrition (e.g., whether there is a bounce in attrition after the payment of the bonus).

The results from the hazard model indicate that, among 5YO_s who accessed in FY93 and FY94 with the promise of an enlistment bonus, increasing the budget for EBs by \$1,000 would produce 1 extra month of service. Of this additional time, 0.8 month would occur after the completion of training and would represent an increase in productive service. These results are roughly consistent with those produced with the binary analysis of attrition: the probit estimates suggested that a \$1,000 increase in the budget for bonuses for these cohorts would yield about 1 month of first-term, post-training work time.

Figure 2 presents survival and hazard functions for 5YO_s who accessed in FY93 and FY94 and who were promised enlistment bonuses. The blue elements of these graphs indicate survival (attrition behavior) based on the enlistment bonuses that the Navy actually promised. The red elements are the survival (attrition) patterns that we predict would result if the Navy were to double its offer of enlistment bonuses (from the mean of \$1,300 that prevailed during this period to \$2,600).

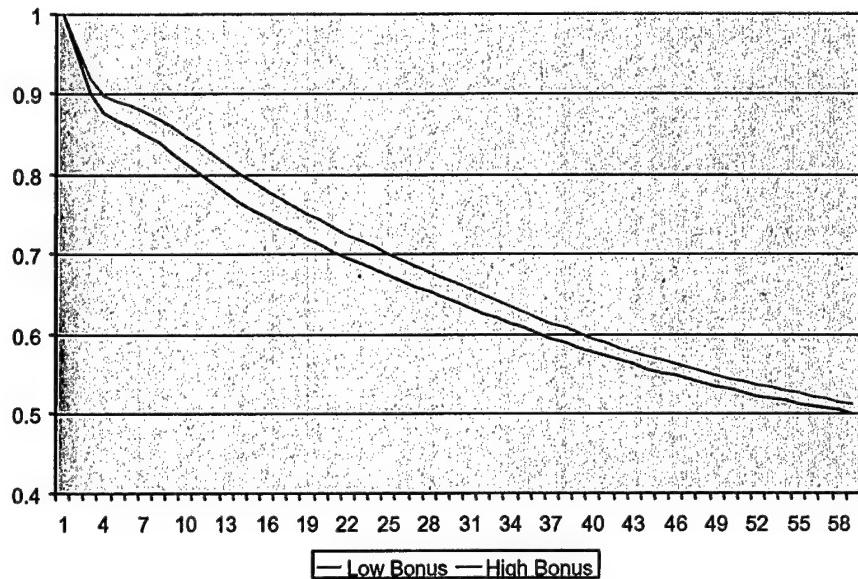
The hazard analysis predicts that increasing the enlistment bonus would sharply reduce attrition for 5YO_s, during both boot camp and training. Moreover, the reduction in early losses would be only slightly offset by greater attrition after the payment of the bonus. We do not observe a large "bounce" in attrition immediately after the payment of the EB; it is apparently not the case that accessions wait until after the payment of the bonus to depart from the service.

Figure 3 shows a similar set of survival and hazard functions for 6YO_s in nonnuclear fields who accessed in FY95. The figures show that, among these cohorts, an additional dollar of bonus would yield a smaller increase in useful service: doubling the enlistment bonus from \$2,500 to \$5,000 would result in an extra 2.1 months, but only about 1 month of this time would be post-training. The hazard model results for these accessions are consistent with those produced by the binary regressions in that both predict that an extra \$1,000 in EB would reduce 24-month attrition by 2 percentage points (from 30 percent to 28 percent).²¹

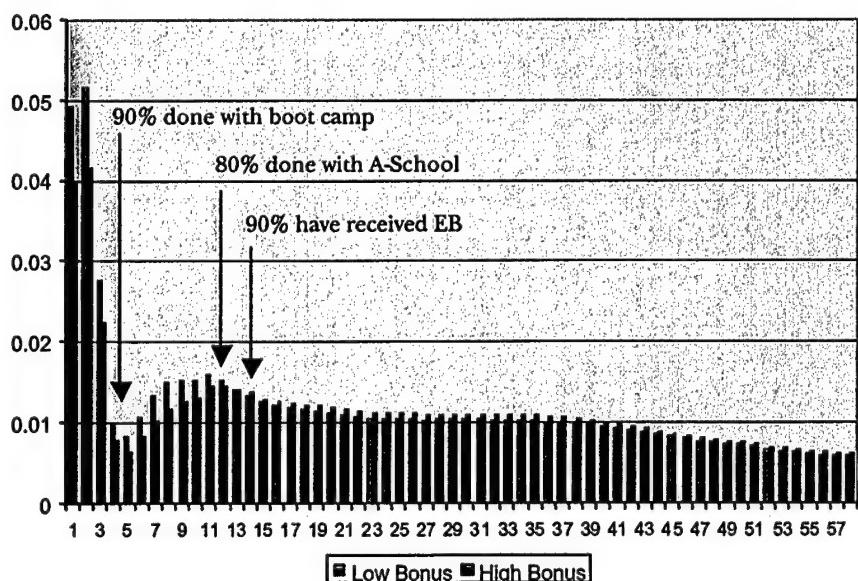
²¹ The hazard data for the 6YO_s who accessed in FY95 extend only through 56 months, rather than through the entire 72 months of their first term. This reflects the fact that our survey data covered only the first part of the cohorts' first term.

Figure 2. Survival among 5YOs

(2a)



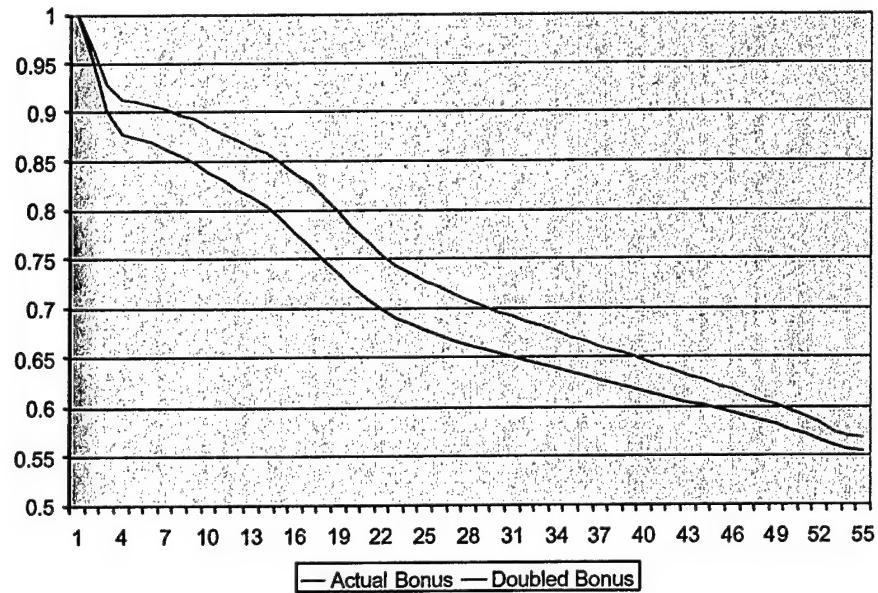
(2b)



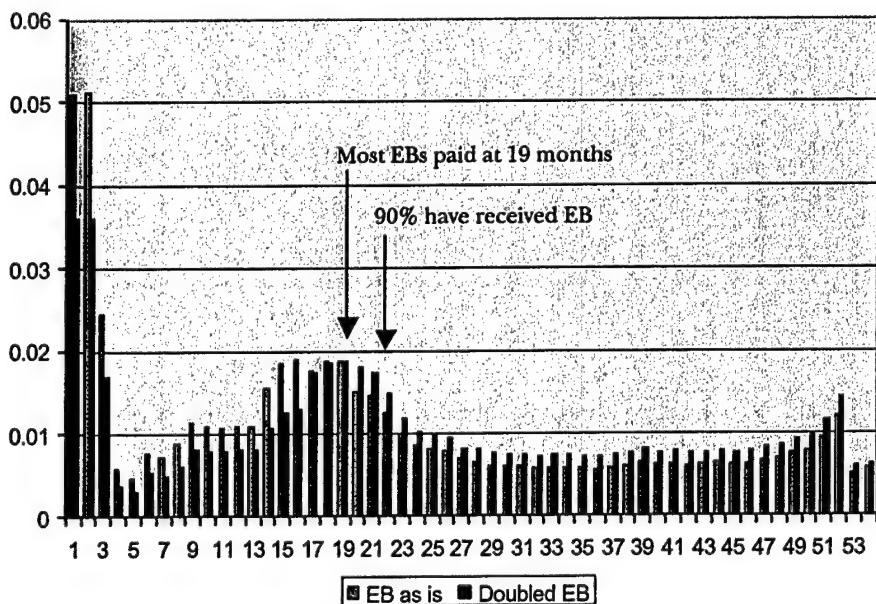
Part (a) shows the survival given the actual bonus for FY93-94 and a doubling of the bonus. Part (b) shows the probability of attrition for each month (conditional on surviving to that month) under the two levels of enlistment bonuses.

Figure 3. Survival among nonnuclear 6YOs

(3a)



(3b)



Part (a) shows the survival given the actual bonus for FY95 and a doubling of the bonus. Part (b) shows the probability of attrition for each month (conditional on surviving to that month) under the two levels of enlistment bonuses.

The need for an experiment

We have observed that two types of estimation bias can hamper analyses of enlistment bonuses. *Selection bias* can arise from larger EBs being promised to personnel who have better than average opportunities in the civilian sector or who have entered ratings that are hard to fill. *Omitted variables bias* could arise if EBs are correlated with unobserved characteristics of the civilian labor market or with unmeasurable traits of accessions.

We have also pointed out that the standard tools for dealing with selection and omitted variables biases are not useful when analyzing the sort of data on EBs that are currently available. In this analysis, we have employed estimators that are biased but that, for some groups of enlistees, can yield *bounds* on the true retention effects of enlistment bonuses. Although we believe that our estimates are the best that could be derived from the existing data, they fall short of providing a complete picture of how EBs affect the service tenure of enlistees.

To produce better estimates of the way EBs affect the career decisions of service personnel, the military would need to conduct a sequel to its original enlistment bonus experiment.²² Under such an experiment, the service would randomly assign *a portion* of the enlistment bonuses to a subset of accessions. This would ensure that part of the bonus would be independent of (uncorrelated with) characteristics of recruits, attributes of ratings, or private-sector labor conditions. With data from such an experiment, one could produce unbiased estimates of how EBs affect the career decisions of service personnel.

A recent report by the General Accounting Office (GAO) suggests that the services have not determined the best mix of resources for recruiting personnel, or the extent to which the branches are competing against each other when they increase the amount spent on

²² Congress mandated a 2-year enlistment bonus experiment when it was considering the introduction of a modest EB program in the early 1980s.

enlistment bonuses and college incentives.²³ An experiment could be designed that would assess both the recruiting and retention effects of enlistment bonuses, evaluate the performance of EBs (in comparison to the Navy College Fund and the Navy College Loan Repayment Program), and answer many other questions about the best way to administer an enlistment bonus program.²⁴ We believe that our statistical results here are sufficiently compelling to justify the Navy pursuing experiments on this issue.

²³ See GAO (2000).

²⁴ For example, such an experiment could help determine the ideal time to pay out the enlistment bonus, and whether retention would be enhanced if EBs were paid to personnel who fail their initial training pipeline but who are reassigned to another technical rating.

Appendix A: Results of the binary regressions

Table 6. A probit of *first-year* attrition among 5YOs

	<u>FY93-94</u>	<u>FY95</u>	<u>FY96-97</u>
Number of observations	2,199	1,913	2,780
LR chi2(9)	64.06	12.30	48.24
Log likelihood	-1,158.93	-980.05	-1,421.90
Dollar value of enlistment bonus	-0.0002 (-2.15)	-0.0002 (-1.32)	0.00002 (0.75)
Age at accession among those in sample	0.0293 (1.31)	0.0158 (0.71)	-0.0065 (-0.36)
Equals 1 if accession is in Tier 1	-0.4874 (-2.17)	-0.3396 (-1.98)	-0.4413 (-3.84)
AFQT of accession	-0.0176 (-5.29)	-0.0053 (-1.51)	-0.0135 (-3.99)
Equals 1 if accession is male	0.2831 (1.71)	-0.0686 (-0.48)	-0.062 (-0.37)
Equals 1 if accession is white	0.3912 (3.06)	0.0450 (0.34)	0.3169 (2.75)
Equals 1 if accession is married	0.5626 (2.44)	0.1412 (0.57)	0.1722 (0.85)
Equals 1 if accession shipped during peak	-0.0686 (-0.63)	-0.0918 (-0.73)	-0.2699 (-2.41)
Equals 1 if accession shipped in FY94	0.0106 (0.10)		
Equals 1 if accession shipped in FY97			-0.1037 (-1.00)
Average unemployment in home state at time of accession	0.0082 (0.22)	-0.0305 (-0.64)	-0.0431 (-1.03)
Constant	-0.6390 (-1.03)	-0.5873 (-0.96)	0.2147 (0.42)
Multiplier	0.18	0.16	0.17

Table 7. A probit of 2-year attrition among nonnuclear 6YO's

	1995	1996	1997
Number of observations	577	2,069	2,536
Log likelihood	-344.75	-1,248.52	-1,580.48
LR chi2(9)	13.56	32.91	72.50
Prob > chi2	0.1390	0.0001	0.0000
Pseudo R2	0.0193	0.0130	0.0224
Dollar value of enlistment bonus	-0.00013 (-2.01)	0.00004 (1.73)	0.00008 (2.95)
Age at accession among those in sample	-0.0040 (-0.11)	-0.0100 (-0.53)	-0.0353 (-2.18)
Equals 1 if accession is in Tier 1	-0.5968 (-2.10)	-0.596 (-4.00)	-0.7279 (-5.83)
AFQT of accession	-0.0056 (-0.61)	-0.0111 (-2.33)	-0.0062 (-1.34)
Equals 1 if accession is male	-0.3559 (-1.30)	-0.3075 (-1.83)	-0.3129 (-2.40)
Equals 1 if accession is white	0.1039 (0.44)	0.2850 (2.25)	0.3653 (3.36)
Equals 1 if accession is married	0.030 (0.08)	0.0438 (0.21)	0.1180 (0.70)
Equals 1 if accession shipped during peak	-0.0727 (-0.36)	-0.0011 (-0.01)	-0.0033 (-0.03)
Average unemployment in home state at time of accession	-0.0544 (-0.71)	-0.0289 (-0.70)	-0.0893 (-2.16)
Constant	1.084 (0.98)	0.7868 (1.34)	1.162 (2.10)
Multiplier	0.18	0.21	0.22

Table 8. A probit of 2-year attrition among nuclear 6YOs

	<u>1993</u>	<u>1994</u>	<u>1995</u>	<u>1996</u>	<u>1997</u>
Number of observations	1,805	2,185	1,935	1,597	1,740
LR chi2(7)	13.01	20.42	51.71	15.81	83.88
<u>Log likelihood</u>	<u>-882.69</u>	<u>-1,102.27</u>	<u>-1,055.90</u>	<u>-919.36</u>	<u>-968.12</u>
Dollar value of enlistment bonus	-0.00005 (-0.55)	-0.00002 (-0.29)	0.00011 (2.25)	0.00002 (0.48)	0.00027 (7.47)
Age at accession among those in sample	-0.1019 (-2.47)	-0.0347 (-0.94)	0.0400 (1.24)	-0.0056 (-0.18)	-0.0338 (-1.04)
AFQT of accession	0.0040 (0.43)	0.0202 (2.38)	0.0038 (0.46)	0.0018 (0.20)	0.0116 (1.38)
Equals 1 if accession is male		-0.8742 (-2.99)	-1.1195 (-6.58)	-0.5185 (-2.80)	-0.3819 (-1.80)
Equals 1 if accession is white	0.3331 (1.68)	0.1634 (0.89)	-0.0928 (-0.61)	-0.0856 (-0.57)	-0.1177 (-0.84)
Equals 1 if accession is married	0.1680 (0.55)	-0.5704 (-1.56)	0.0087 (0.03)	0.3065 (1.21)	-0.4891 (-1.67)
Equals 1 if accession shipped during peak	-0.4186 (-1.80)	0.0187 (0.11)	0.4446 (2.76)	-0.0521 (-0.30)	0.9672 (4.08)
Average unemp't in home state at time of accession	0.0324 (0.76)	0.0052 (0.13)	-0.0646 (-1.42)	-0.0694 (-1.40)	0.0633 (1.25)
Constant	-0.0171 (-0.01)	-1.7619 (-1.13)	-1.4322 (-0.99)	1.0656 (0.91)	-3.9493 (-2.55)
Multiplier	0.12	0.12	0.14	0.14	0.14

Table 9. A probit of *first-year* attrition among 5YOs who accessed in FY93 and FY94 by season of accession

	<u>Peak</u>	<u>Off-peak</u>
Number of obs	1,114	1,085
LR chi2(8)	46.44	33.49
<u>Log likelihood</u>	-578.48	-572.51
Dollar value of enlistment bonus	-0.0009 (-2.20)	-0.00009 (-0.86)
Age at accession among those in sample	0.0756 (2.15)	0.0036 (0.12)
Equals 1 if accession is in Tier 1*	-0.5591 (-1.56)	-0.4527 (-1.56)
AFQT of accession	-0.0189 (-4.00)	-0.0153 (-3.37)
Equals 1 if accession is male	0.1896 (0.80)	0.3889 (1.67)
Equals 1 if accession is white	0.6026 (3.33)	0.2077 (1.13)
Equals 1 if accession is married	-0.1573 (-0.39)	0.9729 (3.35)
Equals 1 if accession shipped in FY94	0.0371 (0.33)	-0.0181 (0.15)
Average unemployment in home state at time of accession	0.0442 (0.83)	-0.0178 (-0.34)
Constant	-0.9960 (-1.03)	-0.2259 (-0.27)
Marginal	0.17	0.18

Appendix B: Signing estimation bias

As explained in the main text, when we regress attrition against the value of the EB, our estimate of the relationship between these variables may be biased. However, we can derive useful information from biased estimates if we can anticipate the likely sign of the estimation bias. “Signing the bias” is possible if we can make the following assumptions about how attrition changes with enlistment bonuses, and about the correlations between enlistment bonuses, economic conditions, and personal characteristics of accessions:

1. Holding constant economic conditions and personal characteristics, a larger enlistment bonus either acts to reduce the level of attrition or has no effect on attrition. This is an innocuous assumption: it simply says that paying people more for their service in the military will not reduce their tenure in the military.
2. Any change in economic conditions that prompts policy-makers to raise (lower) enlistment bonuses will either have an adverse (beneficial) effect on attrition or have no effect on attrition (e.g., a heating-up of the civilian labor market may be associated with both *increased* enlistment bonuses and greater attrition).
3. Holding all else constant, if a larger enlistment bonus expands the market for recruits, those attracted by the larger bonus will be no less likely to attrite than other accessions (i.e., if a larger enlistment bonus is necessary to attract more people from the workforce—people whose initial post-school career choice was not the service—these recruits will not have lower levels of attrition than those who are willing to join the service with a smaller bonus).
4. Ratings that must pay higher bonuses to meet full manning will not have lower levels of attrition than ratings that do not need to offer bonuses. An obvious example is the nuclear field, which, over the study period, had the highest enlistment bonuses but the highest levels of attrition.

Estimation bias arising from misspecification

We make use of these assumptions in the following mathematical argument. Define the variables:

Y = some measure of attrition, perhaps the proportion attriting among a cohort of accessions or a binary variable indicating if an individual attrites.

EB = the dollar value of the enlistment bonus.

X = an $n \times k+1$ matrix composed of n observations on k variables plus an additional column of ones. We construct X such that, when we derive estimated coefficients for each of these variables in an attrition function, our point estimates are expected to be nonnegative. For example, X might include the level of civilian wages and the employment rate (as opposed to the unemployment rate), as neither of these are likely to be negatively correlated with attrition.

ϵ = an error term.

We can write a regression model as

$$Y = X_1\beta_1 + EB\beta_2 + \epsilon \quad (1)$$

where β_1 is a vector of partial correlation coefficients. (The partial correlation coefficient, β_i , indicates how the dependent variable, Y , changes as one of the independent variables, X_i , varies, holding all other independent variables constant.)

The term for the enlistment bonus, EB , can be decomposed into two elements: EB_X , which is orthogonal to the error term, and EB_N , which is correlated with the error term. The error term, in turn, can also be decomposed into two elements: γ , which is correlated with EB_N , and ω , which is orthogonal to EB_N . One can therefore write:

$$Y = X_1\beta_1 + (EB_X + EB_N)\beta_2 + \gamma + \omega. \quad (2)$$

As we will see next, the general formulation shown in equation 2 is especially useful because it allows us to treat many types of endogeneity bias as problems in omitted variables.

Misspecification with grouped data

The problem of misspecification is well known in the military manpower literature and is often discussed in terms of simultaneous determination of retention and compensation. For example, if we were looking at attrition among various cohorts of accessions—our dependent variable might be the proportion of the cohort that attrites—we would need to be concerned that causality between attrition and the size of the enlistment bonus could run in either direction: higher enlistment bonuses might reduce attrition, but greater attrition (or fewer accessions) might prompt policy-makers to raise the size of the enlistment bonus. Such a joint relationship could be represented as follows:

$$\text{Attrition} = X_1 \beta_1 + (\text{EB})\beta_2 + \varepsilon \quad (3)$$

$$\text{Accessions} = X_1 \delta_1 + (\text{EB})\delta_2 + \psi \quad (4)$$

$$\begin{aligned} \text{EB}_t &= W_t \alpha_1 + (\text{Attrition})\alpha_{\text{Attrition}} \\ &\quad + (\text{Accessions})\alpha_{\text{Accessions}} + v_t \end{aligned} \quad (5)$$

Because attrition and the enlistment bonus are jointly determined, the error term in equation 3 is negatively correlated with the regressor EB (the coefficient $\alpha_{\text{Attrition}}$ can be taken as negative). Were one to apply standard regression methods to estimating the coefficient β_2 , such an estimate would be biased and inconsistent. To predict the sign of this bias, we can decompose the error term in equation 3 as done in equation 2:

$$\text{Attrition} = X_1 \beta_1 + (\text{EB})\beta_2 + \gamma + \omega \quad (6)$$

The component of the error term, ω , is uncorrelated with the explanatory variables, while γ is correlated with EB. If we think of γ as arising from some unobserved process, we could express this part of the error term as the product of a missing variable and a partial correlation coefficient, $\gamma = X_3 \beta_3$. We could then rewrite equation 6 as:

$$\text{Attrition} = X_1 \beta_1 + (\text{EB})\beta_2 + X_3 \beta_3 + \omega \quad (7)$$

Writing the attrition equation in this form has some interesting implications. Note first that ω is, by assumption, uncorrelated with any of the regressors in equation 7. This implies that, were one able to

observe the hypothetical variable, X_3 , and to include this as a regressor in an estimate of the attrition function (as represented in equation 7), none of the resulting coefficient estimates would be subject to simultaneity bias.

Also note that, with a little logical deduction, we can assign some real economic meaning to the hypothetical variable, X_3 . This variable is that which must be observed and held constant in order for variation in EB to reflect *only exogenous changes* in the enlistment bonus. This implies that X_3 is equal to the endogenous component of the enlistment bonus—the change in the enlistment bonus that results from a change in X_1 , ω , or EB_X (recall that EB_X is the exogenous component of the enlistment bonus variable). We could, therefore, write $X_3 = EB_N$ (EB_N is the endogenous component of the enlistment bonus variable) and express equation 7 as:

$$\text{Attrition} = X_1 \beta_1 + (EB_X + EB_N)\beta_2 + EB_N \beta_3 + \omega. \quad (8)$$

From assumptions 2 through 4, we know that attrition and the endogenous component of the enlistment bonus have a nonnegative correlation and the coefficient β_3 is nonnegative.²⁵

Since we cannot observe EB_N , we estimate:

$$\text{Attrition} = X_1 \overset{*}{\beta}_1 + (EB_X + EB_N)\overset{*}{\beta}_2 + \omega \quad (9)$$

The usual expression for omitted variables bias in an OLS regression is as follows [see Greene (2000), p. 334]:

$$E[\overset{*}{\beta}_1] = \beta_1 + \frac{\sigma_{11}}{\sigma_{11} + \sigma_{33}} \beta_3 \quad (10)$$

Recalling that we have constructed X_1 such that $\beta_1 < 0$, that $\beta_3 > 0$, and using $\frac{\sigma_{11}}{\sigma_{11} + \sigma_{33}} > 0$, we can conclude that $\overset{*}{\beta}_1 \leq \beta_1$. Using similar arguments, we find that $\overset{*}{\beta}_2 \leq \beta_2$.

²⁵ To help keep this clear, it may be useful to think of EB_X as being randomly assigned and EB_N as being determined by policy-makers. When policy-makers are raising EB_N , it is likely associated with increasing attrition (or falling enlistments) and, therefore, $\beta_3 \geq 0$.

Misspecification with individual data

In the current study, we use individual data to assess the effect of enlistment bonuses on accessions. This means that our dependent variable, Y , is a binary measure that equals 1 if an accession attrites in a specific period, and that is otherwise 0. In this context, we still face a problem of misspecification, although it is *not* generally one that arises from simultaneity bias; to an individual recruit, the enlistment bonus is typically exogenous.

Rather, we face the closely related problem of omitted variables bias. When we regress attrition against the size of the enlistment bonus, there are likely to be variables that we cannot observe, that affect attrition, and that are correlated with the value of the enlistment bonus. If we fail to control for the presence of these omitted variables, we may ascribe their effects on attrition to enlistment bonuses. In the current study, the most obvious candidates for omitted variables are wages in civilian employment (relative to military pay) and the characteristics of recruits that determine whether they will be successful in the military.

Yatchew and Griliches (1984) point out that, when we employ a binary (probit) regression, omitted variables bias is far more complex than that represented in equation 10 above. In fact, it is so complex that one cannot, in general, sign the bias. However, within the context of the current study, we have sufficient restrictions on our estimation that we can sign the bias for a simple model that includes the EB as an explanatory variable and, perhaps, one or two other regressors. To see this, we rewrite the attrition equation as:

$$Y^* = \beta_0 + EB\beta_1 + Z\beta_2 + \omega, \quad (11)$$

where Z is an unobservable variable, such as civilian employment conditions or recruit quality. To keep our argument simple, we assume that the omitted variable is correlated with the level of enlistment bonuses, as represented by the following regression function:

$$Z = \gamma_0 + EB\gamma_1 + v. \quad (12)$$

If one were able to estimate equation 11 with both the observed and unobserved variables, the asymptotic limit of the probit slope parameter would be:

$$b_1 = \beta_1 / \sigma_\omega.$$

However, if one were to estimate the relationship between attrition and enlistment bonuses without including the regressor Z, as in the following misspecified regression equation,

$$Y^* = \beta_0 + EB\beta_1 + \omega, \quad (13)$$

the asymptotic limit of the probit slope parameter would be:

$$b_1^* = \frac{\beta_1 + \gamma_1 \beta_2}{(\beta_2 \sigma_v^2 + \sigma_\omega^2)^{0.5}}. \quad (14)$$

Because of the complexity of this equation, one cannot generally sign $b_1 - b_1^*$. However, we have enough assumptions in place so that we can. Note that $(\beta_2 \sigma_v^2 + \sigma_\omega^2)^{0.5} > \sigma_\omega$, and define $(\beta_2 \sigma_v^2 + \sigma_\omega^2)^{0.5} = \sigma_\omega + \Delta$. So,

$$b_1 - b_1^* = \beta_1 / \sigma_\omega - \frac{\beta_1 + \gamma_1 \beta_2}{\sigma_\omega + \Delta}$$

and

$$\begin{aligned} \text{sign}(b_1 - b_1^*) &= \text{sign}(\beta_1 (\sigma_\omega + \Delta) - (\beta_1 + \gamma_1 \beta_2) \sigma_\omega) \\ &= \text{sign}(\beta_1 \Delta - \gamma_1 \beta_2 \sigma_\omega). \end{aligned} \quad (15)$$

But, Δ and σ_ω are positive, $\beta_1 \leq 0$ by hypothesis, and the product of $\gamma_1 * \beta_2$ is, by assumption, nonnegative.²⁶ Therefore, the sign of expression 15 is nonpositive ($b_1 \leq b_1^*$).

The expression for the asymptotic limit of the probit slope parameter becomes much more complex with each explanatory variable we add to our binary regressions. Moreover, when we add additional regressors, in order to be able to "sign the bias" for the coefficient on the

²⁶ Assumptions 2 to 4 at the beginning of this appendix imply that this product is nonnegative. For example, if the omitted variable is opportunities in civilian employment, this is positively correlated with the size of EBs ($\gamma_1 \geq 0$), and is positively correlated with attrition ($\beta_2 \geq 0$). If the missing variable were civilian *unemployment*, we would have $\gamma_1 \leq 0$ and $\beta_2 \leq 0$.

EB variable, we must be able to impose particular restraints on our estimations. For example, if we were to add an additional regressor to both the attrition equation

$$Y^* = \beta_0 + EB\beta_1 + Z\beta_2 + X\beta_3 + \omega,$$

and to the equation for the missing variable

$$Z = \gamma_0 + EB\gamma_1 + X\gamma_2 + v,$$

we would only be able to sign the bias if we knew, a priori, that the product $\gamma_2 * \beta_2$ were nonnegative.

There are some situations in which this might be reasonable. For example, suppose we are concerned about the impact of unobserved aspects of opportunities in civilian employment and define Z as an unobserved measure of opportunities in the civilian sector. We also define $X = (100 - AFQT)$ as a regressor (a higher number indicates a less intelligent person), and we know, a priori, that X is positively linked with the unobserved measure of opportunities in private employment (when economic conditions improve, we observe lower average intelligence among recruits). Then, it would be reasonable to assume that $\gamma_2 * \beta_2$ is nonnegative.

Appendix C: A discrete-time hazard model²⁷

Although standard statistical approaches can be used for analyzing the occurrence of an event, hazard models are most appropriate when we wish to examine the *timing* of an event. Statistical questions that deal with timing are generally referred to as problems in survival analysis. There are many hazard model specifications for describing survival problems. These typically differ in the assumptions that are made about how the probability of an event changes with time (e.g., does an event remain equally likely over time—such as being hit by lightning—or does the likelihood of an event increase with time—such as the probability of being diagnosed with senile dementia?).

An important historical issue in hazard model analysis is the difficulty in choosing among these various assumptions. When analyzing survival problems, one often has no way to know, *a priori*, how the likelihood of events will change with time. However, one can get very different estimation results from different hazard model specifications. In an important paper, Cox (1972) proposed a method with which one could estimate the effects on survival of various types of explanatory variables (such as economic conditions or the personal characteristics of subjects) without imposing restrictions on how the probability of an event changes with time. In other words, this approach yields results that are not sensitive to the initial assumptions that one makes about how the likelihood of an event changes over time.²⁸

In this paper, we follow the method proposed by Cox (1972), and define a discrete-time hazard:

$$P_{it} = P[T_i = t | T_i \geq t, \mathbf{X}_i], \quad (16)$$

²⁷ Much of this discussion follows Allison (1982).

²⁸ For a description of the statistical characteristics of the Cox model, see Efron (1977) and Kalbfleisch and Prentice (1980).

where T is a discrete random variable giving the time when an event occurs. One can represent the relationship between time and explanatory variables using a logistic regression function:

$$P_{it} = 1/[1 + \exp(-\alpha_i - \beta' X_{it})]. \quad (17)$$

The terms α_i are constants that represent how the likelihood of an event changes with time. These are unspecified in the model, and are estimated in our regression analysis.

The likelihood function for this hazard model can be written:

$$L = \prod_{i=1}^n [P(T_i = t_i)^{\delta_i}] [P(T_i > t_i)]^{1-\delta_i} \quad (18)$$

To estimate this, it is useful to rewrite the individual elements of the likelihood function using conditional probabilities:

$$P(T_i = t_i) = P_{it} \prod_{j=1}^{t-1} [P(1 - P_{ij})] \quad (19)$$

$$P(T_i > t_i) = \prod_{j=t+1}^n (1 - P_{ij}) \quad (20)$$

If we substitute equations 19 and 20 into equation 18, take the log, and do some manipulations, the likelihood function can be rewritten as

$$L = \sum_{i=1}^n \sum_{j=1}^t y_{it} \log[P_{ij}/(1 - P_{ij})] + \sum_{i=1}^n \sum_{j=t+1}^n \log[(1 - P_{ij})] \quad (21)$$

where y_{it} is a binary variable that equals 1 if person i experiences an event at time t , and is otherwise equal to 0. Equation 21 is the standard log likelihood for the regression analysis of binary dependent variables (see Nerlove and Press, 1973).

Brown (1975) was the first to note that discrete-time hazard rate models could be estimated using applications for binary regression models. Allison (1982) summarizes the technique with the following:

In practice, the procedure amounts to this: Each discrete time unit for each individual is treated as a separate observation or unit of analysis. For each of these observations, the dependent variable is coded 1 if an event occurred to that individual in that time unit; otherwise it is coded zero. Thus if an individual experienced an event at time 5, five different observations would be

created. For the fifth observation, the dependent variables would be coded 1. For the other four observations, the dependent variable would be coded zero. The explanatory variables for each of these new observations would be assigned whatever values that they had at that particular unit of time. Lagged values could also be included. To estimate the constants α_t ($t = 1, 2, \dots$), a dummy independent variable would be created for each of the possible time units less 1....The final step is to pool these observations and compute ML estimates.

Abbreviations and acronyms

AFQT	Armed Forces Qualification Test
CNRC	Commander Naval Recruiting Command
DEP	Delayed Entry Program
DoD	Department of Defense
EAOS	End of Active Obligated Service
EB	Enlistment bonus
EM	Electrician's mate
ET	Electronics technician
FY	Fiscal year
GAO	General Accounting Office
MM	Machinist's mate
YO	Years of obligation

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